Inmate Responses to Incentives for Good Behavior

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Abstract

In this paper, we use administrative data from the Oregon Department of Corrections to measure prisoner responses to incentives for good behavior. Namely, we consider two sources of variation, one coming through policy-induced changes in the generosity of sentence reductions available for good behavior, and the other around discontinuous shifts in the expected return to good behavior with every six-month assessment period over which prisoners are awarded their sentence reductions. Our results suggest that prisoner misconduct is largely unresponsive to these incentives.

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1 Introduction

America has a prison problem. In 2008, there were 2.3 million people incarcerated in the United States at an estimated annual cost of 75 billion dollars.[1] Rapid growth in the imprisoned population has also led to significant overcrowding, with recent estimates suggesting that current populations are upwards of 108 percent of capacity.[2] Already, the United States incarcerates more people and a higher percentage of its population than any other country.[3] In fact, Oregon, Vermont, Michigan, Connecticut, and Delaware currently spend more on their prison systems than on higher education; over the past 23 years nationwide prison spending has increased six-times faster than spending on higher education.[4]

While the costs of mass incarceration have attracted recently public attention, a significant literature has suggested crimes prevented through incarcerating prisoners justifies the cost. Overall, it appears that the marginal benefit exceeds the marginal cost for the average prisoner (Levitt, 1996; Owens, 2009; Buonanno and Raphael, 2013) in many settings. However more recent evidence suggests the returns to incarcerating marginal prisoners in the United States may have declined to inefficient levels (Johnson and Raphael, 2012).

One of the principal drivers of the increased incarceration rate have been increased sentence lengths served by prisoners (Raphael and Stoll, 2013). Many factors drove this increase. One significant shift was the adoption of truth-in-sentencing reforms and mandatory minimum punishments. Upon adopting truth-in-sentencing reforms, many states replaced parole boards with "good time", which allowed some prisoners to earn off a pre-determined fraction of their sentence based on their behavior. At the same time, states which retained parole boards often reduced or eliminated the discretion of the parole board, with parole in essence

[1] "The High Budgetary Cost of Incarceration," Center for Economic and Policy Research (June 2010)

[2] "Prison Population Rates per 100,000 of the national population," http://www.prisonstudies.org/info/worldbrief/wpb-stats.php (Jan 2013)

[3] Ibid

[4] "New High in Prison Numbers," The Washington Post (Feb 2008). "When will the U.S. stop mass incarceration?" CNN (July 2012).

become a mandatory event which happened after the prisoner. Currently, 32 states offer some form of "good time," where prisoners' sentences are deterministically reduced as long as the prisoner avoids misconduct citations (Lawrence and Lyons, 2011). The shifts in policy towards earned time have been largely justified that they will lower the costs of incarceration, while also having the potential to contribute to reductions in negative criminogenic effects of incarceration. Furthermore, more generous earned time

The effectiveness of these within-prison deterrence effects have long been assumed by policymakers and voters with good time policies standing alongside only community corrections as correctional policies that have received broad support from the public (Skovron, Scott, and Cullen, 1988). As Larkin (2013) suggests, "Good-time laws never have been as politically volatile with the electorate, and have never generated the same visceral, adverse reaction from the public as have the parole laws ... Perhaps that is because the availability of good-time credit was universally accepted as a necessary tool for wardens to prevent institutions from becoming a Hobbesian state of nature." Despite the strong public support for good time policies, there is very little empirical evidence about the relationship between good-time policies and prisoner misconduct rates.[5] Whether sentence-reduction policies are effect in actuality depends largely on the deterability of inmates (Drago, Galbiati, and Vertova, 2009; Blumstein, Cohen, and Nagin, 1978; Hansen, Forthcoming). We seek to understand how individuals, who incidentally were not deterred from committing crimes based on existing enforcement levels and punishments, respond to the deterrent incentives of assessment cycles.

Shifting to more-generous "good time" has recently attracted media coverage, as the role of earned time has intersected with the nationwide problem of mass-incarceration and prison overcrowding. Indeed, more-generous good time could theoretically improve prisoner behavior while incarcerated and thereby reduce costs. With movements to increase good time

[5]More research exists in the consideration of prison-administrators' perceptions of good-time policies. In general, surveyed prison officials feel that good-time policies are important to maintaining control of prisons (Ross and Barker, 1986).

available at the federal level (e.g., The Barber Amendment would double the federal good time earned), Oregon's recent good-time modifications, driven by budgetary considerations, provide a unique quasi-experiment to assess whether the incentives offered by good time shift prisoner behavior while they are still behind bars. Between the 2009 and 2013, Oregon shifted the amount of good time prisoners could earn on four occasions, alternating between more- and less-generous good time. We examine unique administrative records on prisoner behavior and misconducts over this window. In addition, good time was awarded over six-month intervals, thereby enabling both the shifts in good-time generosity and potential change in prisoner behaviour over the assessment cycles to determine whether inmates are responsive to those incentives for good behavior.

The remainder of the paper proceeds as follows. In Section 2, we detail the policy variation we exploit for identification, as well as the manner in which prisoners can earn time off their sentence. In Section 3, we discuss the data and methodology, presenting our main results in Section 4. We consider the review cycles themselves in Section 5, and offer concluding remarks in Section 6.

2 Background

Federal and state sentencing practices have experienced fundamental shift toward truth in sentencing over the last 20 years. While early innovators—Oregon among them—often abandoned parole boards altogether in favor of determinate sentencing, the particular regimes states entered into varied, and continue to vary over time. For example, truth in sentencing generally implies that convicts serve the sentences assigned to them, but sentence reductions can and are often made available to prisoners in exchange for prescribed good behavior.

When parole was abandoned in Oregon in 1989, the model that replaced it allowed for sentence-length reductions of up to 20 percent. While this is accurately characterized as a reward for good behavior, the sentence reductions have traditionally been framed as a

punishment for bad behavior. In fact, prisoners are informed upon entry that they should expect to receive all available sentence reductions and thereby exit prison at 80 percent of their maximum sentence.[6] While the 20-percent rule stood in place for some time in Oregon, this policy has been changed several times in recent years, largely motivated by budgetary concerns. It is these regime changes we exploit for identification, following several administrative rule changes that increased sentence reductions from 20 to 30 percent for some crimes, later reversed this ruling, only to reinstated the 30-percent rule again for a smaller subset of crimes. In addition, every six months prisoners have an evaluation of their misconducts and any associated losses are determined and are thereafter irrevocable. It is prisoner behavior around these six-month reviews—particularly major misconducts—that is the subject of our analysis.[7]

The incentives to behave while in prison may also effect recidivism and future crime by reducing the criminogenic effects of prison. Both Chen and Shapiro (2007) and Drago, Galbiati, and Vertova (2011) find that more-secure prisons with relatively harsh conditions lead to increases in post-release crime. A potential mechanism for this effect is the increased misconducts prisoners experience in prisons with higher security levels. Further evidence suggests that the criminogenic effects of prison lead to significant increases in post-release crime relative to criminals who were not incarcerated (Di Tella and Schargrodsky, 2013; Nieuwbeerta, Nagin, and Blokland, 2009).

The consistent finding that prison time leads to increased future crime has a number of potential explanations including criminal-network development (Bayer, Hjalmarsson, and Pozen, 2009) and the development of norms that favor crime (Trulson, Caudill, Haerle, and

[6]This type of framing causes the sentence reductions for good behavior to be viewed by inmates as punishments for bad behavior. Bushway and Owens (2013) finds that framing can significantly alter criminal behavior, with perceived punishment severity reducing recidivism.

[7]Sentence reductions are not available to prisoners convicted of certain violent crimes which have mandatory minimum punishments (also referred to as "Measure 11" offenses). Measure 11 offenders still experience behavioral reviews in six-month intervals. In practice Oregon continues to incentivize these prisoners with privileges such as preferred housing, visitation, and other privileges which may be removed following an unfavorable review.

DeLisi, 2012). In addition, there is significant evidence suggesting that misconducts while incarcerated are predictive of future crime (Cochran, Mears, Bales, and Stewart, 2012). This implies that reductions in misconduct rates may yield long-term benefits through decreasing criminogenic effects.

In a meta-analysis of 39 studies, Gendreau, Goggin, and Law (1997) finds that both personal characteristics such as risk preferences and situational factors including prison security level could be used to predict misconduct rates. In addition, prison systems often do internal analyses to improve their own ability to predict misconducts. In Oregon, for example, incoming prisoners are assigned a "violence-predictor score" based on the prisoner's age, gender, prior incarcerations, type of crime, aggression level, drug history, and personality disorders (if any). This score is then used to determine the likelihood that the prisoner commits violent misconducts in their first year of incarceration and thereby contributes to determining the appropriate security level for their incarceration. One important element not included in these evaluations is the prisoner's eligibility for parole and/or deterministic-sentence reductions. This omission is noteworthy due to the strong evidence that prisoner's serving sentences without eligibility for parole commit significantly more misconducts than do their parole-eligible peers (Bales and Miller, 2012).

3 Data and Methods

3.1 Data

All data come from the administrative records of the Oregon Department of Corrections, inclusive of prisoner characteristics at admission and high-frequency information about misconducts, activities, and the timing of prisoner assessment and their outcomes.[8] Our sample used for analysis includes all adult-male inmates who committed crimes on or after 1 July

[8]The information regarding prisoner characteristics at admission include the inmates' age, race, criminal history (number of convictions and types), education, conviction dates, and offense date.

2009 but before 1 July 2013. We observe misconducts for this sample for the portion of sentences served between 1 July 2009 and (at the time of writing) 10 July 2014. Our first-order interest will be to estimate the effect of the changes to sentence-reduction policy on prisoners' propensities to commit misconducts. As only major misconducts determine sentence reductions—major misconducts account for 94 percent of all misconducts—we will limit out attention to major misconducts and choose to drop the those above the 99^{th} percentile. In Table 2 we report summary statistics, where we also group crimes into categories that will reflect the policy experiments we follow.

In Figure 1 we depict the policy-driven variation in available sentence reductions. Within our sample period, the sentence-reduction regime a prison falls into is determined by the crime committed and the date on which the crime occurred.[9] While evidence of judicial discretion is present within the data, we rely on the identifying variation that exists across time within a given category of crime. We we group crimes into four categories following the administrative rules related to sentence reduction. The most-severe crimes are never eligible for sentence reductions.[10] Prisoners having committed Group B crimes experience 20-percent sentence reductions throughout the period of our analysis. It is the other two groups that experience policy shocks directly; one experiencing a one-time change in available reduction and the other experiencing the same change only to to be reversed 16 months later.[11]

A delay in conviction following crime commission is expected. This difference is larger for those with violent or sex-related crimes, for example, and shorter among this with drug-

[9]We assume the most-severe crime a prisoner is convicted of determines sentence-reduction treatment at the prisoner level.

[10]The ineligibility of these prisoners was established in Oregon by Measure 11. This policy, enacted in 1994 and later expanded to include more crimes excludes specific severe crimes from sentence reduction eligibility although in some cases judges are given discretion to allow for sentence reduction eligibility at a 20-percent rate. A complete list of crimes that are not eligible for sentence reductions of any kind can be found in Table 1.

[11]For those convicted before 1 July 2009, judge discretion determined whether they transitioned to 30-percent reduction in 2009. Not observing judges' determinations, we are not able to exploit within-prisoner variation for identification. Ultimately, we discard all prisoner-day observations associated with crimes committed before 1 July 2009.

related crimes. This raises suspicion that variation in this difference may also move systematically with unobservables. In our analysis, we exclude all prisoners committing crimes after the most-recent policy change of 1 July 2013. By doing so, we capture more than 99 percent of the crimes that have led or will lead to convictions during our sample.

3.2 Methods

Our first approach to identifying the causal effect of sentence-reduction generosity is to exploit policy-induced time-series variation in available reductions to identify whether there are changes in misconduct rates in Oregon prisons. In particular, we will estimate RD models of the sort,

$$M_{it} = \alpha + \beta 1(CrimeDate_t > d) + \theta CrimeDate_t + \psi CrimeDate_t 1(CrimeDate_t > c) + \epsilon_i t, \tag{1}$$

where M_{it} is the number of major misconducts committed on day t by prisoners i, and β captures the treatment effect of 30-percent sentence reductions on misconducts. As usual, this model measures the local average treatment effect by considering the difference in the estimated conditional expectations of M_{it} on each side of the treatment threshold. For example, around the 17 February 2010 regime change,

$$\lim_{r \uparrow c} \mathbb{E}[M_{it} \mid CrimeDate_i = 17Feb2010] - \lim_{r \downarrow c} \mathbb{E}[M_{it} \mid CrimeDate_{it} = 1Feb2010]. \tag{2}$$

In preferred specifications, we will also include a set of variables that flexibly control for the prisoner characteristics including number of total and violent convictions, age, race, sentence length, days served up to that point, ten categories of crime, and facility fixed effects for both the facility the prisoner was initially assigned to and the facility they were ultimately released from (or the facility they reside in at the end of our sample if they have yet to be

released).[12] In estimating standard errors we allow for clustering at the crime-date level.

In Figure 2 we see the evidence of the regime changes, which will serve as the source of exogenous variation we exploit for identification in subsequent analysis. In Table 3 we confirm the existence of a first stage econometrically for prisoners in groups C and D around 17 February 2010 and, in Table 4, for Group D prisoners around 1 July 2011.

3.3 Standard RD-Validation Checks

Before continuing to consider rates of misconduct around the treatment thresholds available for identification, we first pause to establish that observable characteristics and the distribution of the running variable are smooth around these thresholds. While it may be surprising to see in corrections data, violating these smoothness assumptions is usually taken as evidence that there is manipulation of the running variable. In Table 5 we consider whether observable characteristics are smooth through the threshold, raising no surprises and supporting the legitimacy of our methods. In Figure 3 we follow McCrary (2008) to further confirm that there is no discontinuity in the distribution across the treatment threshold. Thus, we proceed to anticipate that the estimated parameters retrieved from our regression-discontinuity design will facilitate making causal inference.

4 Results

In Figure 4 we see a visual representation of the RD estimates, with crime dates gathered in ten-day bins. Only among Group C prisoners is there the appearance of a discontinuity in misconduct rates associated with treatment—lower rates on the "treatment" side of the threshold, where 30-percent sentence reductions are available.

[12]These crime categories are violent crimes, drug-related crimes, white collar crimes, theft, parole violation, vandalism, gun-related crimes, child-sex crimes, sex related crimes, and then a category for all others.

In Table 6 we present the simplest of our specifications, separately allowing for quadratic trends on either side of the treatment threshold and identifying any discontinuity in misconduct rates among Group C prisoners with the 17 February 2010 policy change. Though somewhat imprecisely measured, around this policy experiment, there is no apparent change in misconduct rates across treatment and control regimes. In Column (2) we add prisoner controls and in Column (3) we further add facility fixed effects. In no specification can one conclude that sentence-reduction generocity influences misconduct rates in a significant way. We repeat this analysis for Group D prisoners around the two regime changes such prisoners experienced on 17 February 2010 and 1 July 2011, with results reported in tables 7 and 8. Again, there is no evidence of systematic improvement in behavior coincident with more-generous sentence reductions.

4.1 Robustness and Heterogeneity Analysis

In tables 12 through 15 we further explore the potential for available sentence reductions to contribute to rates of prisoner misconduct by stratifying across prisoner age, a prior of each prisoner's likelihood of recidivism, education, and race, for each of the three regime changes.[13] Although some point estimates are large in magnitude, representing sizable effect sizes, in no case do we find significant changes in prisoner misconduct around treatment.

In tables 16 through we consider (for each policy experiment) the potential for non-linearities in treatment across sentence served. Specifically, we allow for the effect of sentence reduction generosity on major misconducts to vary across the first 30 days served, second thirty, etc.. Again, there is no such response evidence in major misconducts, or in drug misconducts, violent misconducts, or when misconducts are separated by whether they involved single or multiple prisoners.

[13]On entry, all Oregon prisoners are assigned an Automated Criminal Risk Score (ACRS) to identify offenders most likely to recidivate.

5 Six-Month-Review Cycles

5.1 Background

The administrative-review cycles for prisoner incentives provide several predictions assuming Beckerian models of deterrence. Early in the review cycle, prisoners should commit more misconducts because the expected returns to behaving well on a particular day are lower due to the number of future days on which a prisoner also has to behave well in order to earn sentence reductions. Likewise, later in the review cycles inmates should commit fewer misconducts due to the decreased interval over which they must avoid misconducts. This implies that if we were able to control for other potentially confounding factors, the number of misconducts should be positively related to the number of days from the next review. Furthermore, there should also be a jump in misconduct rates at the start of a new review cycle due to the discontinuity in deterrence around the assessment period. As a preliminary analysis, then, we first estimate whether the number of days until a subsequent review is positively related to the number of misconducts.

The discontinuous incentive structure around the end of review cycles naturally lends itself to a regression discontinuity model as one approach to identifying whether the review cycles alter prisoner behavior—the estimated discontinuity reflects the degree to which misconduct rates tend to vary between the first and last days of the average review period. First introduced by (Thistlethwaite and Campbell, 1960), regression discontinuity (RD) offers a useful approach to identify the causal effect of treatments when treatment status is determined by a discontinuity in another variable. In our case, the variable that determines treatment is the days from review. In order for an RD to produce unbiased estimates, any variation in either observable or unobservable characteristics should remain smooth through the threshold where the discontinuity occurs (Hahn, Todd, and Van der Klaauw, 2001).

The main threat to these assumptions in our case will be the timing of when inmates leave prison. As shown in Figure 5, there is substantial variation in assigned sentence length, and

possibly some heaping in particular sentence lengths, which might challenge identification if any such discontinuities in the density of the running variable (McCrary, 2008) reveal an underlying non-random selection out of the sample around a review period. However, when we examine a histogram the number of days since inmates entered prison, as in Figure 6, there is no evidence of discontinuous exit patterns.[14] To ensure that exit issues do not arise, we restrict our attention to prisoners with adjacent six-months reviews. While this causes abrupt decreases in the "days served" histogram, it creates a perfectly balanced, uniform density when we rescale the number of days individuals have served around the thresholds. With no exit from the sample, by construction, the density is uniform across the threshold and the relevant density tests (McCrary (2008) and Frandsen (2013)) therefore raise no concerns.

5.2 Results

In this section we consider whether there is any systematic discontinuity in prisoner misconducts coincident with what we have argued is a discontinuity in each prisoner's incentives on the day of assessment. However, here we do so without the same potential for confoundedness that arises in being able to separately identify the within-assessment-period slope parameter and other trends in misconduct rates (e.g., prisoner-specific rates falling across time, aggregate rates tending to fall across time). Again, assuming they have behaved well up until that point, prisoners who are one-day shy of their next evaluation only have to behave well for one additional day to earn their entire available sentence reductions for that period. However, on the day following an assessment, in order to earn the reward prisoners must forecast behaving well for that and all remaining days until their next assessment. Due

[14]This likely happens become of quasi-random variation in the amount of "time-served" inmates have upon entered prison depending on their trial length and whether they were originally jail, and variation due to the earned sentence reductions themselves. In addition, many prisoners in our sample have not-yet completed their sentence. In these cases the maximum value of days served is simply the difference between the last day of our sample and the day the prisoner entered prison.

to the uncertainty surrounding their ability to behave well for the entire six-month span, the expected returns to behaving well should be much higher on the day just prior to the assessment than on the day immediately following an assessment–it is this discontinuity that we exploit for identification.

In so doing, we construct "synthetic" assessment periods that begin 89 days prior to the day of assessment and last until 89 days after assessment.[15] In two related analyses, in Sections 5.2.1 and 5.2.2, we separately consider the potential discontinuity in misconduct rates coincident with assessment.

5.2.1 Identifying changes in misconduct rates around assessment

Let M_{dap} be counts of major misconducts on day d in synthetic-assessment period a of prisoner p. Days are organized for each prisoner in relation to his day of evaluation, so $d = -1$ is the day before prisoner p's evaluation, $d = 0$ is the day of prisoner p's evaluation, and so on; d ranges from -89 to 89 Thus, we define $DFA_{dap} \in [-89, 89]$ as the days from assessment. The econometric model is therefore of the form,

$$\begin{aligned} M_{dap} &= \alpha + \gamma_1 1(DFA_{dap} \geq 0) + \gamma_2 DFA_{dap} \\ &\quad + \gamma_3 DFA_{dap} \times 1(DFA_{dap} \geq 0) + \mu_{dap}, \end{aligned} \tag{3}$$

where μ_{dap} is a random error term. In (3), the local average treatment effect, $\hat{\gamma}_1$, is identified by considering the difference in the estimated conditional expectations of M_{dmp} on each side of the treatment threshold,

$$\lim_{r \uparrow 0} \mathbb{E}[M_{dap} \mid DFA_{dap} = r] - \lim_{r \downarrow 0} \mathbb{E}[M_{dap} \mid DFA_{dap} = r]. \tag{4}$$

[15] Six month review cycles last between 180 and 184 days including the day of the review. In order to ensure that the sample of prisoners on each side of the review is identical, we impose a maximum bandwidth of 89 days in either direction and consider smaller bandwidths as part of a subsequent sensitivity analysis.

In the context of the traditional regression-discontinuity design, observations for which $DFR_{dap} \geq 0$ are therefore "treated," with observations for which $DFR_{dap} < 0$ serving as the control, together allowing us to retrieve an estimate of the change in average misconduct rates across the discontinuity.

In Table 19 we first reproduce $\hat{\gamma}_1$ from (3) among all prisoners eligible for sentence reductions. Then, in subsequent columns, we reproduce $\hat{\gamma}_1$ after adding controls (i.e., indicators of number of convictions, number of violent convictions, age decile, race, sentence-length decile crime type, day of week, month, and year) in Column (2), facility by month fixed effects, as prisoner behavior could vary systematically across facilities (e.g., through guard behavior) in Column (3), and facility by day-of-week fixed effects in Column (4). Largely invariant to choice of specification, estimates in columns (1) through (4) suggest that daily misconduct rates do no change in the period following review.

In Table 20 we estimate models identical to column (4) of Table 19, separately for prisoners in each crime group. While all groups of inmates have incentives to behave well around review cycles—even prisoners ineligible for sentence reductions face potential reductions in privileges like visitation and phone use in the six-month reviews—the incentives are much stronger for the prisoners who are eligible for sentencing reductions. As in the previous section, we find no evidence that any group of prisoners responds to the discontinuous change to incentives to behave well coincident with review.

5.2.2 Short-term responses to assessment

To isolate the short-term misconduct effect of evaluation from other factors, we follow Stephens (2003) and Evans and Moore (2011) in estimating an econometric model similar to (3) but with additional flexibility on either side of the evaluation day. That is, allowing for greater flexibility in the days around the threshold itself, we model,

$$M_{dap} = \alpha + \beta_d \sum_{d=-15}^{15} DFA_{dap} + \delta X_{dap} + \mu_{dap}, \tag{5}$$

where we allow for separate intercept shifters, β_d, for each day within 15 days of review. Thus, each of the 31 $\hat{\beta}_d$ identify the degree to which rates of misconduct on day d differ systematically from those in the $[-89, -16]$ and $[16, 89]$ ranges. As in previous models, we also include flexible prisoner and time controls as well as facility fixed effects.

In Figure 7 we plot all β_d estimates from (5), which has the potential to reveal any empirical regularity in misconduct rates not attributable to controls. These figures therefore reveal day-specific departures from the estimated means on each side of the assessment and, consistent with Table 19, there does not appear to be a general decrease in misconducts leading up to the assessment. On the other hand, Figure 7, does reveal two days on which there are significant improvements in behaviour: the days immediately before and after assessment.

This improvement in behavior on the day prior to assessment is consistent with models of inmate myopia. This has been observed in other settings, with McCrary and Lee (2009) finding evidence that teens show relatively small responses to the increase in punishments arising when individuals reach adulthood. Such a response however would remain consistent with a Beckerian model of crime, with some individuals exhibiting quasi-hyperbolic discounting. However, there is also a disproportionate decrease in misconducts the day follow an assessment, which is not predicted in models of deterrence. It is instead consistent with models of reinforcement, where success at an assessment may temporarily encourage inmates to continue their improved behavior.[16] That said, if this type of reinforcement is driving the reduction in misconducts following assessment, the effect appears to be short lived.[17]

[16]Given that misconducts in prison are rare events, occurring on only 0.3 percent of prisoner days, 90 percent of prisoner-review cycles result in a full award of sentence reductions. Among prisoners who are penalized in a review we see a similar drop off in misconducts on the day immediately following the review. This may be the result of short term penalties that limit misconduct opportunities for prisoners (e.g., solitary confinement). Conversely, it may suggest that the short term behavioral improvement resulting from an evaluation does not depend on the evaluation resulting in maximum sentence reductions.

[17]It is noteworthy that this reinforcement effect occurs both among prisoners who earn all available sentence reductions in a given period and those who are penalized. This suggests that both positive and negative reinforcement can improve behavior in the short term.

When analyzing the day-specific response of prisoners by their eligibility to receive sentence reductions, in Figure 8, the estimates suggest prisoners eligible for sentence reductions exhibit the larger decrease in misconducts on the day prior to their assessment. This is notable as they have the largest potential benefits to a successful review. However, both types of prisoners show signs of potential reenforcement.

In summary, the results suggest that prisoners show signs of responding to assessment cycles consistent with models of deterrence. The inmates with the greatest incentives to respond to the assessment cycles—those eligible for sentencing reductions—show the greatest responses. However, we also find particular improvements in inmate behavior in the (single) day immediately following an assessment, suggesting that other behavioral elements are in play beyond deterrence.

6 Conclusion

In this paper we analyze the responsiveness of prisoners to specific behavioral incentives. Specifically, we examine how inmates respond to the discontinuous change in incentives arising at the end (and beginning) of regular and repeating assessment periods. Consistent with the predictions of models of deterrence, we find that prisoner behavior is significantly worse immediately following an assessment. We also find that inmates entitled to sentence-length reductions in response to their ability to avoid major misconducts are the most responsive, while those ineligible to receive sentence reductions exhibit more-muted responses. We also find evidence to support myopia, as prisoners improve their behavior in the days immediately prior to and immediately following an assessment.

These results have important policy implications. They suggest that inmates are responsive to the incentives associated with sentence reductions being available, which should lead to less-costly incarceration while also reducing the potential criminogenic effects of incarceration. Without the same incentives, those prisoners ineligible for sentencing reductions

do not respond similarly around assessment cycles. While the lack of behavioral incentives offered to this group may be justified on punitive or incapacitation grounds, these limitations may be significantly reducing the overall effectiveness of sentence reductions as a policy tool. This will hold particularly true if sentence reductions are not extended to those prisoners most likely to commit misconducts.

The evidence we find suggests that a more-effective strategy may be to assign prisoners to longer sentences to begin with (maintaining punitive and incapacitation effects) but then allow these prisoners to earn sentence reductions through good behavior. Such policies may offer significant cost savings to prisons and, to the extent that misconducts in prison encourage new crimes upon release, improve the rehabilitation objectives of corrections systems.

Finally, it is important to consider whether the behavioral changes we observe within prison translate into behavior outside of corrections. That is to say, we do not know if prisoners who are less incentivized to behave well while in prison are more likely to commit crimes once released. Whether the generosity of sentence reductions is effective in reducing the criminogenic effects of incarceration is also an important consideration, as is an understanding the implications of these policy changes on recidivism.

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7 Tables and Figures

Figure 1: Sentence-Reduction Maximums, by Crime and Date Committed

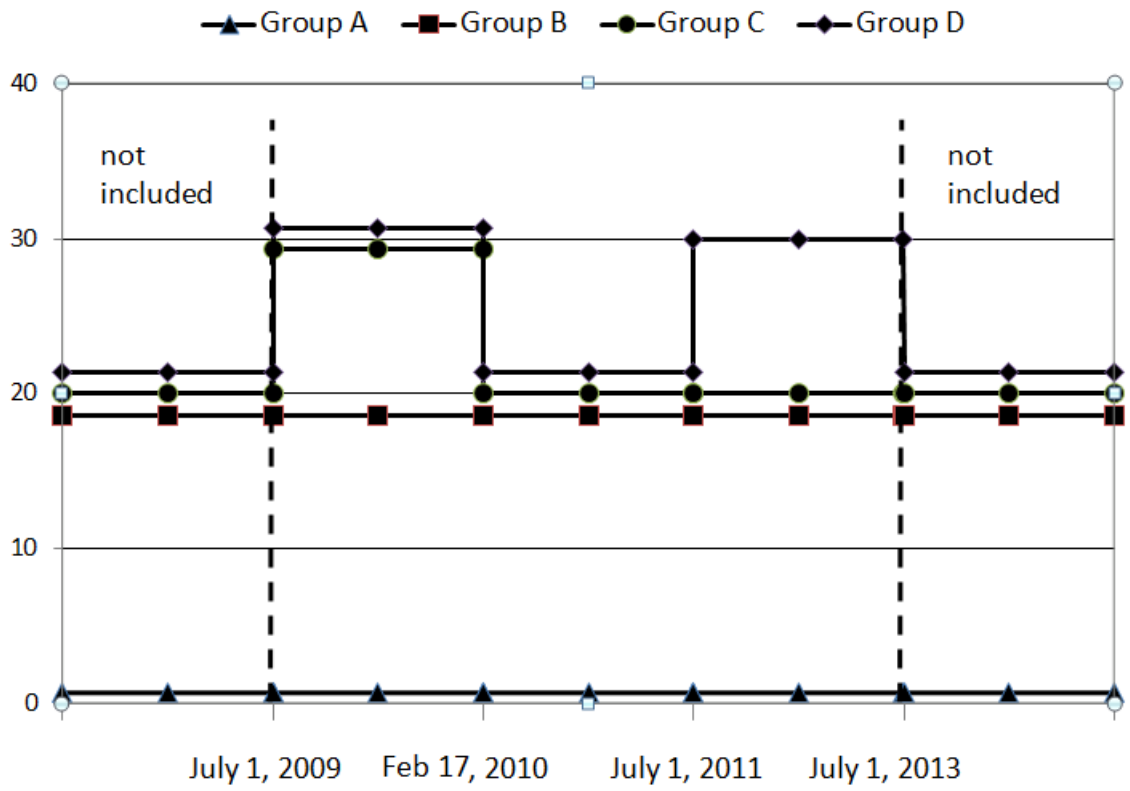


Table 1: Crimes, Grouped According to Sentence-Reduction Regimes (Roughly Corresponding to Severity)

Group A Crimes: Sentence reductions only by judge discretion

No judge discretion to award sentence reductions

Murder	Rape I	Assault I
Arson I	Rape II	Display Child Sex
Kidnapping	Sexual Abuse I	Sodomy I
Kidnapping II	Manslaughter I	Robbery

Judge discretion permits 20-percent sentence reductions

Assault II	Manslaughter II	Robbery II
Unlawful Sexual Penetration I	Unlawful Sexual Penetration II	
Sodomy II	Compelling Prostitution	

Group B Crimes: 20% sentence reductions available throughout sample period

Assault III	Criminally Negligent Homicide	Sex Abuse II
Assault IV	Rape III	Sodomy III

Group C Crimes: 30% sentence reductions available if committed between 1 July 2009 and 17 Feb 2010; 20% thereafter.

Abandon Child	Abuse Of Corpse I & II	Aggravated Animal Abuse I
Aggravated Vehicular Homicide	Animal Abuse C Felony	Assault Law Enforcement Animal
Assault Public Safety Officer	Attempted Weapon Use	Unlawful Burglary I
Buy/Sell A Minor	Child Neglect I	Coercion
Cause Person To Ingest Dangerous Substance	Criminal Mistreatment I	Custodial Sexual Misconduct I
Driving Under Influence Felony	Encouraging Child Sex Abuse I	Encouraging Child Sex
Abuse II	Encouraging Child Sex Abuse III	Escape I
Firearm - Pointing At Another	Firearm Used In Felony	Harassment Aggravated
Hit Run With Injury	Incest	Intimidation I
Involuntary Servitude I	Luring A Minor	Maintaining Dangerous Dog
Online Sex Corrupt Child I & II	Pay To View Child Pornography	Poss Of Hoax Destructive Device
Possess Child Porn Material I, II, & III	Possess Child Pornography	Possession Body Armor
Prostitution Promotion	Public Indecency	Racketeer Activity
Robbery III	Sexual Assault Of Animal	Contribute to Sexual Delinquency of a Minor
Sexual Misconduct	Stalking Felony	Strangulation Felony
Supply Contraband	Theft By Extortion	Theft I Aggravated
Unlawful Contact With A Child	Use Mace, Tear Gas, or Stun Gun	Weapon Possession - Inmate
Weapon Use Unlawful		

Group D Crimes: 30% sentence reductions available if committed between 1 July 2009 and 17 Feb 2010, or after 1 July 2013; 20% elsewhere.

(All crimes not in groups A, B, or C.)

Notes: Attempting to commit any of these crimes also qualifies them in the same category.

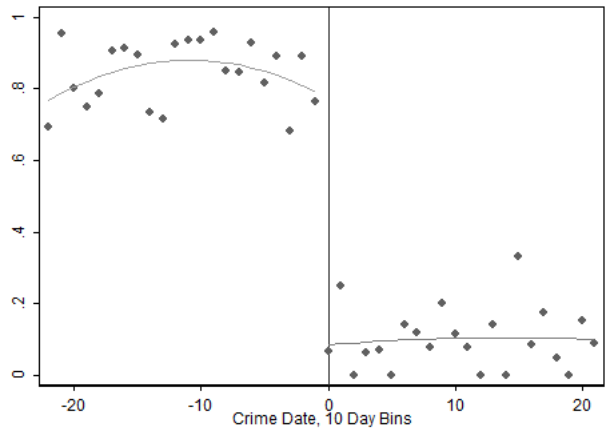
Table 2: Summary Statistics

	All Prisoners	Group A	Group B	Group C	Group D
Major Misconducts	2.32	4.46	1.89	2.45	2.04
Drug Misconducts	0.11	0.23	0.22	0.33	0.30
Violent Misconducts	0.33	0.66	0.07	0.10	.011
Single-Person Misconducts	1.34	2.80	0.93	1.34	1.20
Multi-Person Misconducts	2.73	4.79	2.19	2.99	2.40
Fraction of Time Earned	0.90	0.84	0.93	0.90	0.91
Fraction Lost for Misconducts	0.07	0.13	0.05	0.07	0.06
Days from Crime to Conviction	173.84	203.20	234.80	176.46	162.73
Total Crime Convictions	1.88	3.42	1.79	1.94	1.67
Violent Crime Convictions	0.35	1.05	0.87	0.26	0.26
Sentence Length	910.52	2,189.92	778.71	862.24	794.49
Age	35.50	34.48	31.90	35.41	36.03
Max Days Served (by 02/28/2015)	621.92	1012.62	627.94	647.52	560.84
White	0.74	0.78	0.77	0.77	0.73
Black	0.09	0.08	0.07	0.10	0.09
Hispanic	0.15	0.11	0.13	0.10	0.15
Other Race	0.03	0.03	0.03	0.03	0.03
ACRS Score	0.27	0.23	0.19	0.24	0.30
Recidivists	0.41	0.59	0.26	0.33	0.44
Parole Violators	0.22	0.41	0.14	0.19	0.23
Prisoners	7,369	511	431	2,195	4,231

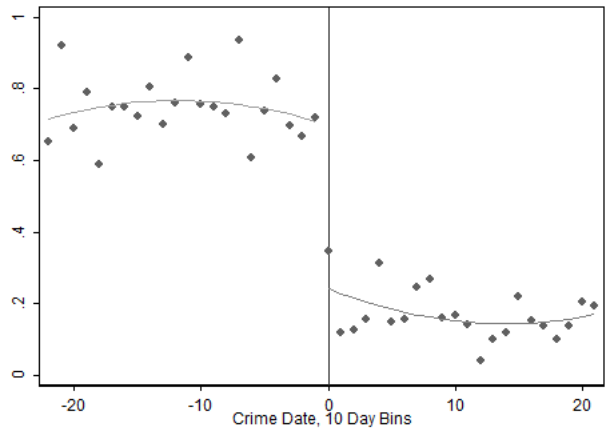
Notes: Group A includes only prisoners convicted of crimes that made them ineligible for sentence reductions. The All eligible category includes all prisoners not in group A. Group B includes only prisoners convicted of crimes that were eligible for 20-percent sentence reductions regardless of the date the crime was committed. Group C includes prisoners convicted of crimes that were eligible for sentence reductions of 30 percent if he crime was committed before 17 February 2010. Group D includes prisoners convicted of crimes that were eligible for 30-percent sentence reductions if committed before 17 February 2010 or after 1 July 2011.

Figure 2: Policy-Induced Variation in Available Sentence Reductions, by Crime Group

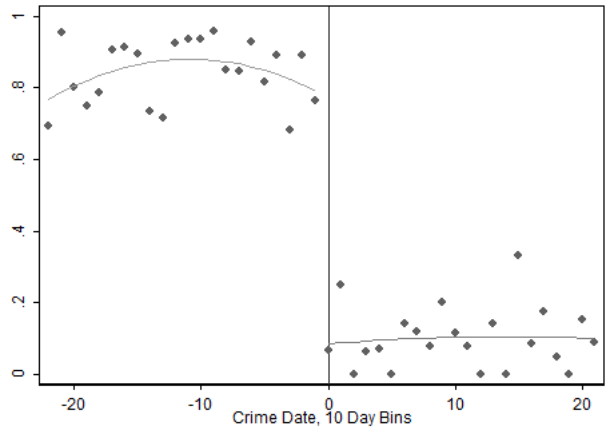
RD: Group C, 17 Feb 2010



RD: Group D, 17 Feb 2010



RD: Group D, 1 July 2011



Notes: Here

Table 3: Are prisoners more likely to be found on 30 percent after 17 Feb 2010?

	All Prisoners	Group A	Group B	Group C	Group D
	(1)	(2)	(3)	(4)	(5)
Treatment (intended)	0.49471***	0.15908	-0.00525	0.68807***	0.46529***
	(0.05119)	(0.24711)	(0.20314)	(0.07188)	(0.07211)
Crime Date	-0.00055	-0.00090	-0.00159	0.00055	-0.00117
	(0.00070)	(0.00352)	(0.00169)	(0.00097)	(0.00103)
(Crime Date)^2	0.00000	0.00001	0.00001	-0.00000	0.00000
	(0.00000)	(0.00002)	(0.00001)	(0.00000)	(0.00000)
Crime Date × Treatment	-0.00045	-0.00030	0.00024	-0.00254*	0.00002
	(0.00107)	(0.00542)	(0.00395)	(0.00148)	(0.00152)
(Crime Date × Treatment)^2	-0.00001	-0.00001	-0.00001	-0.00001	-0.00001
	(0.00000)	(0.00002)	(0.00002)	(0.00001)	(0.00001)
Observations	2575	148	176	780	1471

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. t-statistics in parentheses. "Treatment" equal to one for those crimes committed before 17 February 2010, and therefore intended to move to 30-percent.

Table 4: Are prisoners more likely to be found on 30 percent after 1 July 2011?

	All Prisoners	Group A	Group B	Group C	Group D
	(1)	(2)	(3)	(4)	(5)
Treatment (intended)	0.28280***	0.10691	-0.07905	0.17660***	0.40169***
	(0.03568)	(0.12577)	(0.07312)	(0.05781)	(0.05011)
Crime Date	0.00039*	0.00079	0.00025	0.00008	0.00048
	(0.00020)	(0.00081)	(0.00054)	(0.00028)	(0.00029)
(Crime Date)^2	0.00000**	0.00000	0.00000	0.00000	0.00000*
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Crime Date × Treatment	-0.00037	-0.00167	0.00091	-0.00018	-0.00040
	(0.00033)	(0.00117)	(0.00080)	(0.00053)	(0.00045)
(Crime Date × Treatment)^2	-0.00000	0.00000	-0.00000	0.00000	-0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Observations	5980	434	326	1773	3447

Notes: * p $\leq$ 0.1, ** p $\leq$ 0.05, *** p $\leq$ 0.01. t-statistics in parentheses. "Treatment" equal to one for those crimes committed after 1 July 2011, and therefore intended to move to 30-percent.

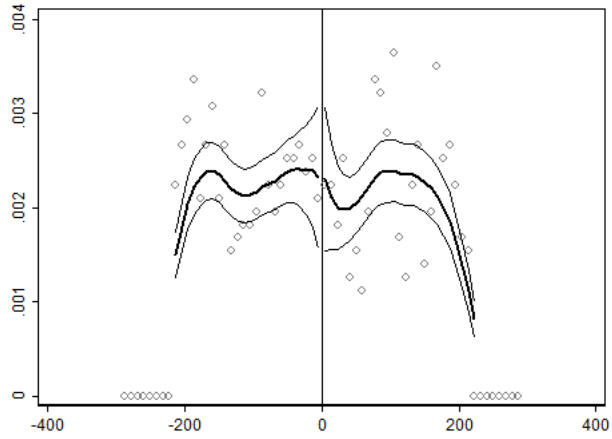
Table 5: Covariate Smoothness Across Treatment Thresholds, by Treatment Episode and Crime Group

	RD: 17 February 2010			RD: 1 July 2011		
	Group B	Group C	Group D	Group B	Group C	Group D
Days from Crime to Conviction	-22.81	34.16	15.11	-36.64	16.22	-3.21
	(46.56)	(21.10)	(14.38)	(34.92)	(14.54)	(9.75)
Total Crime Convictions	0.62	0.26	0.43**	-0.33	-0.11	0.06
	(0.46)	(0.35)	(0.19)	(0.41)	(0.23)	(0.12)
Violent Crime Convictions	-0.21	0.19**	0.04	-0.23	0.10*	-0.05
	(0.21)	(0.08)	(0.06)	(0.22)	(0.05)	(0.04)
Sentence Length	94.14	102.54	12.85	-55.25	-43.52	76.68*
	(133.71)	(126.57)	(102.47)	(59.91)	(42.24)	(41.20)
Age	2.37	-0.68	1.19	0.60	-2.32***	-1.32***
	(0.49)	(1.69)	(1.09)	(1.18)	(0.54)	(0.36)
White	0.12	-0.07	0.01	-0.04	0.04**	0.01
	(0.12)	(0.06)	(0.05)	(0.05)	(0.02)	(0.01)
Black	-0.03	0.07	-0.00	0.04	0.01	-0.00
	(0.07)	(0.04)	(0.03)	(0.03)	(0.01)	(0.01)
Hispanic	-0.14	0.01	0.01	-0.00	-0.05***	-0.01
	(0.10)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)
ACRS Score	0.03	0.03	0.03	0.01	0.02*	0.01
	(0.04)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Recidivists	0.14	0.05	0.03	0.08	-0.02	-0.03*
	(0.12)	(0.07)	(0.05)	(0.05)	(0.02)	(0.02)
Parole Violators	-0.13	-0.01	-0.02	-0.00	-0.03	-0.03*
	(0.10)	(0.06)	(0.04)	(0.04)	(0.02)	(0.01)
Observations	194	837	1,588	329	1,781	3,450

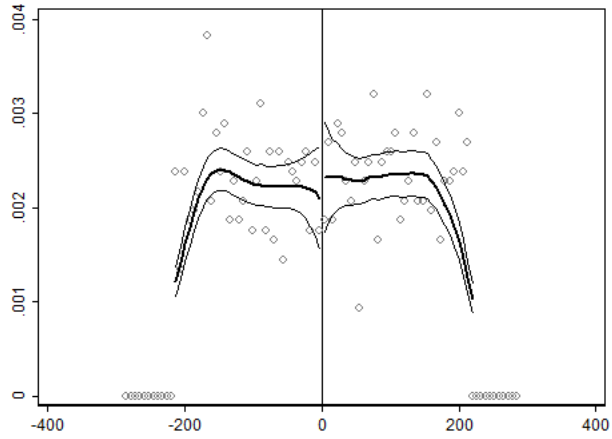
Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Each cell reports the estimated coefficient of a separate regression of treatment (to 30-percent sentence reduction) on the covariate and a crime-date trend as the only independent variables. Robust standard errors reported in parentheses.

Figure 3: Is There Sorting Across the Threshold (McCrary, 2008)

RD: Group C Prisoners, 17 Feb 2010



RD: Group D Prisoners, 17 Feb 2010



RD: Group D Prisoners, 1 July 2011

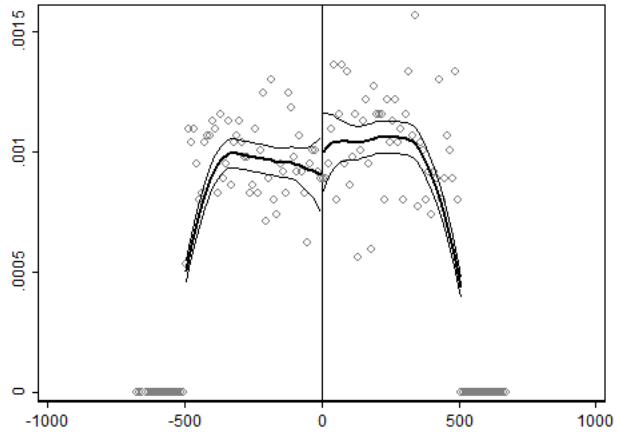


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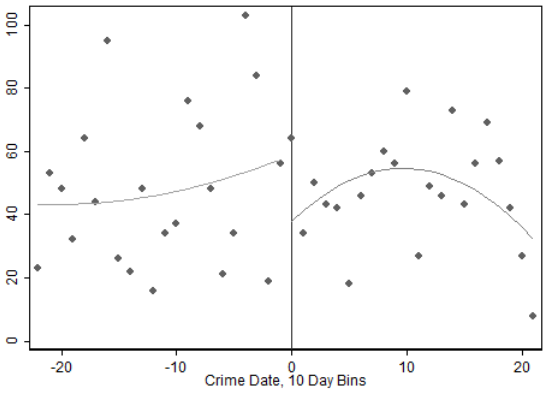
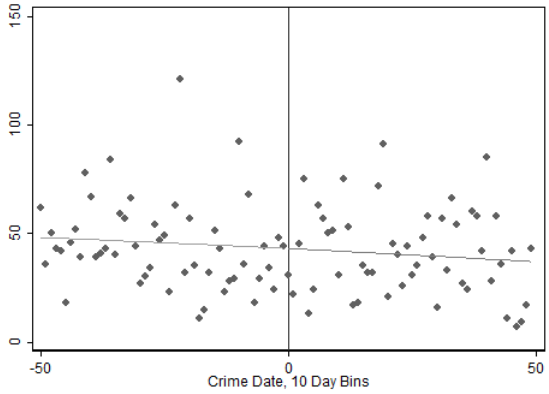
Figure 4: RD Plots by Group and Policy Change

Panel A: Major Misconducts, Group C Prisoners

RD: 17 Feb 2010

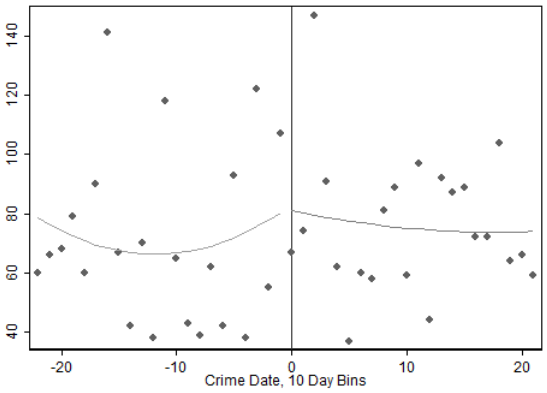


RD: 1 July 2011 (falsification)

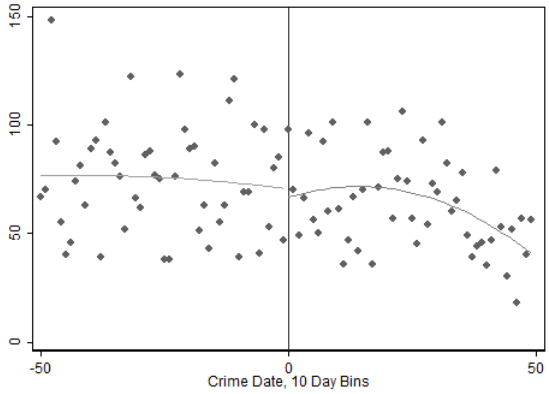


Panel B: Major Misconducts, Group D Prisoners

RD: 17 Feb 2010



RD: 1 July 2011



Notes: Here

Table 6: Major Misconducts, Group C Prisoners, 17 Feb 2010

	(1)	(2)	(3)
30-Percent	0.08078	-0.07216	0.24001
	(1.22399)	(1.14809)	(1.06546)
Crime Date	0.00312	0.00375	0.00466
	(0.01670)	(0.01580)	(0.01406)
(Crime Date)^2	-0.00003	-0.00003	-0.00003
	(0.00007)	(0.00006)	(0.00006)
Crime Date × Treatment	-0.00201	-0.00535	0.00075
	(0.02316)	(0.02029)	(0.01851)
(Crime Date × Treatment)^2	0.00001	0.00002	0.00005
	(0.00009)	(0.00009)	(0.00008)
Conviction Date - Crime Date		-0.00331***	-0.00224***
		(0.00085)	(0.00077)
Days Served		0.00828***	0.00368*
		(0.00214)	(0.00216)
(Days Served)^2		-0.00000	-0.00000
		(0.00000)	(0.00000)
Observations	780	780	780
Mean Misconducts	2.79	2.79	2.79
Prisoner Controls	No	Yes	Yes
Facility FE	No	No	Yes

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parentheses. The sample includes all Group C prisoners who committed crimes between 1 July 2009 and 21 September 2010 (+/- 217 of the policy change), other than those in the top percentile of total major misconducts commit throughout prisoner sentences (i.e., more than 33). Controls include number of total and violent convictions, age, race, sentence length, and shifters for violent crimes, drug-related crimes, white-collar crimes, theft, parole violation, vandalism, gun-related crimes, child-sex crimes, and sex-related crimes.

Table 7: Major Misconducts, Group D Prisoners, 17 Feb 2010

	(1)	(2)	(3)
30-Percent	0.13441	0.46656	0.22067
	(0.73959)	(0.65767)	(0.64315)
Crime Date	-0.00772	-0.00362	-0.00944
	(0.01049)	(0.00973)	(0.00946)
(Crime Date)^2	0.00003	0.00002	0.00004
	(0.00004)	(0.00004)	(0.00004)
Crime Date × Treatment	0.02344	0.01906	0.02543**
	(0.01561)	(0.01350)	(0.01286)
(Crime Date × Treatment)^2	0.00004	0.00005	0.00003
	(0.00007)	(0.00006)	(0.00006)
Conviction Date - Crime Date		-0.00201***	-0.00122**
		(0.00056)	(0.00051)
Days Served		0.00551***	0.00455***
		(0.00166)	(0.00156)
(Days Served)^2		-0.00000	-0.00000
		(0.00000)	(0.00000)
Observations	1472	1472	1472
Mean Misconducts	2.20	2.20	2.20
Prisoner Controls	No	Yes	Yes
Facility FE	No	No	Yes

Notes: * p ≤ 0.1, ** p ≤ 0.05, *** p ≤ 0.01. Standard errors allow for clustering at the crime-date level and are reported in parentheses. The sample includes all Group D prisoners who committed crimes between 1 July 2009 and 21 September 2010 (+/- 217 of the policy change), other than those in the top percentile of total major misconducts commit throughout prisoner sentences (i.e., more than 33). Controls include number of total and violent convictions, age, race, sentence length, and shifters for violent crimes, drug-related crimes, white-collar crimes, theft, parole violation, vandalism, gun-related crimes, child-sex crimes, and sex-related crimes.

Table 8: Major Misconducts, Group D Prisoners, 1 July 2011

	(1)	(2)	(3)
30-Percent	-0.37672	-0.31005	-0.27273
	(0.44554)	(0.39584)	(0.38812)
Crime Date	0.00043	0.00130	0.00168
	(0.00291)	(0.00266)	(0.00258)
(Crime Date)^2	0.00000	0.00000	0.00000
	(0.00001)	(0.00001)	(0.00000)
(Crime Date $\times$ Treatment)	0.00090	-0.00148	-0.00289
	(0.00401)	(0.00355)	(0.00346)
(Crime Date $\times$ Treatment)^2	-0.00001	-0.00000	-0.00000
	(0.00001)	(0.00001)	(0.00001)
Conviction Date - Crime Date		-0.00148***	-0.00165***
		(0.00035)	(0.00034)
Days Served		0.00518***	0.00406***
		(0.00128)	(0.00124)
(Days Served)^2		-0.00000	-0.00000
		(0.00000)	(0.00000)
Observations	3447	3447	3447
Mean Misconducts	1.79	1.79	1.79
Prisoner Controls	No	Yes	Yes
Facility FE	No	No	Yes

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parentheses. The sample includes all Group D prisoners who committed crimes between 17 February 2010 and 10 November 2012 (+/- 499 of the policy change), other than those in the top percentile of total major misconducts commit throughout prisoner sentences (i.e., more than 33). Controls include number of total and violent convictions, age, race, sentence length, and shifters for violent crimes, drug-related crimes, white-collar crimes, theft, parole violation, vandalism, gun-related crimes, child-sex crimes, and sex-related crimes.

Table 9: Major Misconducts, Group C Prisoners: 17 Feb 2010 Extensive Margin

	(1)	(2)	(3)
30-Percent	-0.06160	-0.06906	-0.03202
	(0.10086)	(0.09744)	(0.09115)
Crime Date	0.00006	-0.00045	-0.00031
	(0.00146)	(0.00146)	(0.00131)
(Crime Date)^2	-0.00000	-0.00000	-0.00000
	(0.00001)	(0.00001)	(0.00001)
Crime Date $\times$ Treatment	-0.00005	0.00049	0.00118
	(0.00213)	(0.00210)	(0.00197)
(Crime Date $\times$ Treatment)^2	0.00001	0.00000	0.00001
	(0.00001)	(0.00001)	(0.00001)
Conviction Date - Crime Date		-0.00035***	-0.00024**
		(0.00012)	(0.00011)
Days Served		0.00104***	0.00047**
		(0.00020)	(0.00020)
(Days Served)^2		-0.00000***	-0.00000
		(0.00000)	(0.00000)
Observations	780	780	780
Mean Misconduct (0-1)	0.50	0.50	0.50
Prisoner Controls	No	Yes	Yes
Facility FE	No	No	Yes

Notes: * p $\leq$ 0.1, ** p $\leq$ 0.05, *** p $\leq$ 0.01. Standard errors allow for clustering at the crime-date level and are reported in parentheses. The sample includes all Group D prisoners who committed crimes between 1 July 2009 and 21 September 2010 (+/- 217 of the policy change), other than those in the top percentile of total major misconducts commit throughout prisoner sentences (i.e., more than 33). Controls include number of total and violent convictions, age, race, sentence length, and shifters for violent crimes, drug-related crimes, white-collar crimes, theft, parole violation, vandalism, gun-related crimes, child-sex crimes, and sex-related crimes.

Table 10: Major Misconducts, Group D Prisoners: 17 Feb 2010 Extensive Margin

	(1)	(2)	(3)
30-Percent	-0.03263	0.02627	0.00849
	(0.07559)	(0.07522)	(0.07495)
Crime Date	-0.00157	-0.00078	-0.00130
	(0.00126)	(0.00122)	(0.00121)
(Crime Date)^2	0.00001	0.00000	0.00001
	(0.00001)	(0.00001)	(0.00001)
Crime Date × Treatment	0.00256	0.00179	0.00250
	(0.00171)	(0.00162)	(0.00160)
(Crime Date × Treatment)^2	-0.00000	0.00000	-0.00000
	(0.00001)	(0.00001)	(0.00001)
Conviction Date - Crime Date		-0.00027***	-0.00020**
		(0.00008)	(0.00008)
Days Served		0.00122***	0.00115***
		(0.00018)	(0.00019)
(Days Served)^2		-0.00000***	-0.00000***
		(0.00000)	(0.00000)
Observations	1471	1471	1471
Mean Misconduct (0-1)	0.49	0.49	0.49
Prisoner Controls	No	Yes	Yes
Facility FE	No	No	Yes

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parentheses. The sample includes all Group D prisoners who committed crimes between 1 July 2009 and 21 September 2010 (+/- 217 of the policy change), other than those in the top percentile of total major misconducts commit throughout prisoner sentences (i.e., more than 33). Controls include number of total and violent convictions, age, race, sentence length, and shifters for violent crimes, drug-related crimes, white-collar crimes, theft, parole violation, vandalism, gun-related crimes, child-sex crimes, and sex-related crimes.

Table 11: Major Misconducts, Group D Prisoners: 1 July 2011 Extensive Margin

	(1)	(2)	(3)
30-Percent	-0.04042	-0.02108	-0.01757
	(0.04998)	(0.04755)	(0.04652)
Crime Date	0.00006	0.00010	0.00012
	(0.00035)	(0.00034)	(0.00034)
(Crime Date)^2	-0.00000	-0.00000	-0.00000
	(0.00000)	(0.00000)	(0.00000)
Crime Date $\times$ Treatment	-0.00022	-0.00055	-0.00066
	(0.00046)	(0.00045)	(0.00044)
(Crime Date $\times$ Treatment)^2	-0.00000	0.00000	0.00000
	(0.00000)	(0.00000)	(0.00000)
Conviction Date - Crime Date		-0.00024***	-0.00024***
		(0.00006)	(0.00006)
Days Served		0.00113***	0.00102***
		(0.00012)	(0.00013)
(Days Served)^2		-0.00000***	-0.00000***
		(0.00000)	(0.00000)
Observations	3447	3447	3447
Mean Misconduct (0-1)	0.51	0.51	0.51
Prisoner Controls	No	Yes	Yes
Facility FE	No	No	Yes

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parentheses. The sample includes all Group D prisoners who committed crimes between 17 February 2010 and 10 November 2012 (+/- 499 of the policy change), other than those in the top percentile of total major misconducts commit throughout prisoner sentences (i.e., more than 33). Controls include number of total and violent convictions, age, race, sentence length, and shifters for violent crimes, drug-related crimes, white-collar crimes, theft, parole violation, vandalism, gun-related crimes, child-sex crimes, and sex-related crimes.

Table 12: Heterogeneity: Prisoner Age

	RD: 17 February 2010		RD: 1 July 2011
	Group C	Group D	Group D
	(1)	(2)	(3)
Prisoners 26 and younger			
30-Percent	0.85116	3.39738	-0.55823
	(3.41698)	(2.10558)	(1.34445)
Observations	202	269	725
Mean Misconducts	5.42	3.71	4.21
Prisoners in [27,33]			
30-Percent	2.00620	-0.36461	-1.55333**
	(1.90354)	(1.25992)	(0.66387)
Observations	182	394	935
Mean Misconducts	2.88	2.46	2.52
Prisoners in [34,43]			
30-Percent	-2.54614***	-1.10934	0.66929
	(0.95970)	(0.84939)	(0.51011)
Observations	189	409	931
Mean Misconducts	1.85	2.18	1.76
Prisoners 44 and older			
30-Percent	0.10512	-0.30443	-0.07353
	(0.83737)	(1.01513)	(0.60219)
Observations	191	369	779
Mean Misconducts	1.07	1.17	1.17

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parenthesis.

Table 13: Heterogeneity: ACRS Scores

	RD: 17 February 2010		RD: 1 July 2011
	Group C	Group D	Group D
	(1)	(2)	(3)
1^{st}-quartile ACRS Scores			
30-Percent	0.33937	0.76806	0.35866
	(1.62974)	(1.44714)	(0.85424)
Observations	124	150	326
Mean Misconducts	1.35	1.73	1.52
2^{nd}-quartile ACRS Scores			
30-Percent	-1.88433	1.03449	0.09565
	(3.28267)	(1.08912)	(0.84848)
Observations	190	325	658
Mean Misconducts	3.42	2.78	2.36
3^{rd}-quartile ACRS Scores			
30-Percent	-1.63600	0.32320	-1.23492
	(1.61388)	(1.04249)	(0.82159)
Observations	273	407	963
Mean Misconducts	3.01	2.33	2.57
4^{th}-quartile ACRS Scores			
30-Percent	0.66199	0.66022	0.06223
	(1.44175)	(0.96101)	(0.52529)
Observations	193	589	1500
Mean Misconducts	2.70	1.99	2.15

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parenthesis.

Table 14: Heterogeneity: Education

	RD: 17 February 2010		RD: 1 July 2011
	Group C	Group D	Group D
	(1)	(2)	(3)
GED or Less Education			
30-Perent	-2.43681	0.45119	-0.42021
	(1.78726)	(0.93690)	(0.76464)
Observations	195	312	814
Mean Misconducts	1.87	1.90	2.02
HSD or More Education			
(mean) regime30_first	0.81429	0.34042	-0.19119
	(1.29573)	(0.78686)	(0.44440)
Observations	585	1159	2633
Mean Misconducts	3.04	2.32	2.32

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parenthesis.

Table 15: Heterogeneity: Race

	RD: 17 February 2010		RD: 1 July 2011
	Group C	Group D	Group D
	(1)	(2)	(3)
White Prisoners			
30-Percent	0.25996	0.70689	-0.27942
	(0.94351)	(0.79306)	(0.41448)
Observations	582	1059	2551
Mean Misconducts	2.47	2.17	2.24
Black Prisoners			
30-Percent	-7.04090	1.07718	0.92109
	(6.02912)	(2.17900)	(1.75996)
Observations	82	126	310
Mean Misconducts	3.67	2.67	2.53
Hispanic Prisoners			
30-Percent	7.90165	0.62126	-1.06985
	(4.77607)	(1.85729)	(0.79545)
Observations	90	238	480
Mean Misconducts	2.13	2.23	2.16

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parenthesis.

Table 16: Misconducts, Group C: 17 Feb 2010 Extensive Margin Across Days Served

	0 - 30	31 - 60	61 - 90	91 - 180	0 - 180
	(1)	(2)	(3)	(4)	(5)
Major Misconducts	0.04130	-0.06587	-0.01448	0.00971	-0.01028
	(0.04861)	(0.06795)	(0.07803)	(0.08491)	(0.10742)
Mean	0.04	0.07	0.09	0.19	0.30
Drug Misconducts	0.02293	-0.01293	0.00637	-0.02520	-0.01251
	(0.01873)	(0.01637)	(0.02225)	(0.03150)	(0.03707)
Violent Misconducts	0.04036	0.02017	-0.03810	0.04217	0.08001
	(0.02681)	(0.03566)	(0.04173)	(0.05535)	(0.06695)
Single-Person Misconducts	0.04029*	-0.02998	0.02553	0.09765	0.02297
	(0.02156)	(0.03927)	(0.05395)	(0.06982)	(0.09429)
Multi-Person Misconducts	0.04094	-0.03766	-0.01622	-0.05016	-0.04484
	(0.04842)	(0.06134)	(0.06274)	(0.07702)	(0.10055)

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parentheses. Each cell reports the estimated coefficient of a separate regression that captures the propensity to observe non-zero misconduct counts for given categories of misconduct and within samples restricted to each prisoners' first 30 days, second 30 days, etc..

Table 17: Misconducts, Group D: 17 Feb 2010 Extensive Margin Across Days Served

	0 - 30	31 - 60	61 - 90	91 - 180	0 - 180
	(1)	(2)	(3)	(4)	(5)
Major Misconducts	-0.02693	-0.04112	0.00550	-0.05512	-0.10753
	(0.04387)	(0.04484)	(0.03987)	(0.06384)	(0.06594)
Mean	0.04	0.07	0.08	0.21	0.31
Drug Misconducts	0.00723	0.00174	-0.00744	-0.01200	0.00732
	(0.00461)	(0.00346)	(0.01006)	(0.02036)	(0.02372)
Violent Misconducts	0.00336	-0.02774	0.00129	-0.00504	-0.02596
	(0.02471)	(0.03254)	(0.01334)	(0.03404)	(0.04516)
Single Person Misconducts	-0.00434	-0.01037	-0.02225	-0.05200	-.08085*
	(0.02213)	(0.02308)	(0.02464)	(0.04484)	(0.04809)
Multi Person Misconducts	-0.00777	-0.02758	0.01054	0.00136	-0.03886
	(0.03719)	(0.04313)	(0.03435)	(0.05665)	(0.06454)

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parentheses. Each cell reports the estimated coefficient of a separate regression that captures the propensity to observe non-zero misconduct counts for given categories of misconduct and within samples restricted to each prisoners' first 30 days, second 30 days, etc..

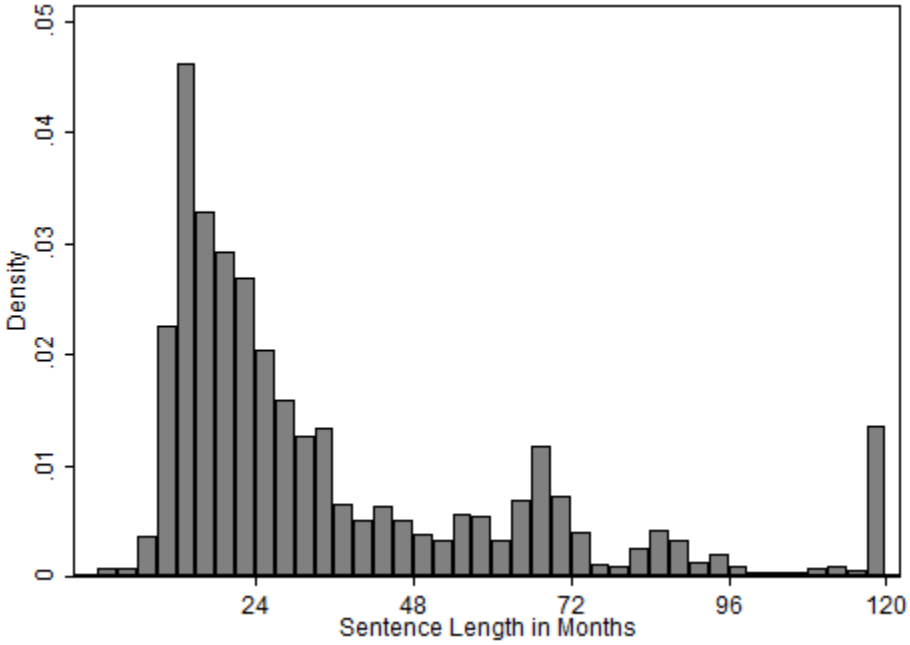
Table 18: Misconducts, Group D: 1 July 2011 Extensive Margin Across Days Served

	0 - 30	31 - 60	61 - 90	91 - 180	0 - 180
	(1)	(2)	(3)	(4)	(5)
Major Misconducts	0.00985	-0.01361	-0.02281	0.01627	0.00337
	(0.02134)	(0.02739)	(0.02886)	(0.04090)	(0.04594)
Mean	0.05	0.07	0.07	0.19	0.30
Drug Misconducts	0.00136	-0.01044	-0.00433	-0.00844	-0.01559
	(0.00600)	(0.00908)	(0.00666)	(0.01638)	(0.01951)
Violent Misconducts	0.01063	-0.01195	-0.00178	0.03154	0.01446
	(0.01451)	(0.01334)	(0.01831)	(0.02096)	(0.02887)
Single Person Misconducts	-0.01322	-0.00339	-0.00617	-0.03923	-0.06147*
	(0.01499)	(0.01743)	(0.01697)	(0.02619)	(0.03308)
Multi Person Misconducts	0.01002	-0.00780	-0.01990	-0.00979	-0.01880
	(0.01994)	(0.02288)	(0.02577)	(0.03625)	(0.04190)

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the crime-date level and are reported in parentheses. Each cell reports the estimated coefficient of a separate regression that captures the propensity to observe non-zero misconduct counts for given categories of misconduct and within samples restricted to each prisoners' first 30 days, second 30 days, etc..

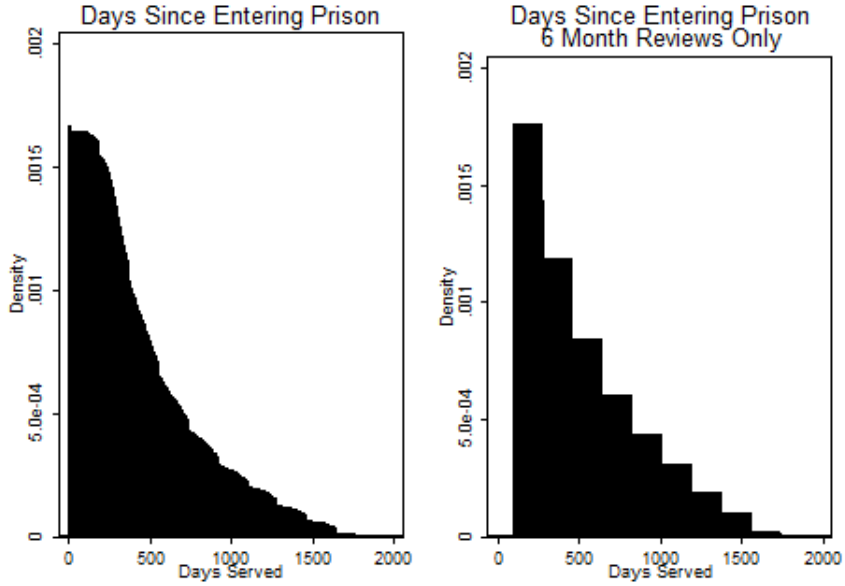
7.1 Review Cycles

Figure 5: Sentence Lengths



Notes: Results based on the population of male prisoners convicted of crimes in Oregon committed after June 30, 2009 and before July 1, 2013. We include all prisoner-days from entry until the earlier of the prisoner's release and June 30, 2014. All sentence lengths longer than 120 months were top-coded to 120 months. These sentences represent the maximum number of days a prisoner could serve if they are not convicted of additional crimes while incarcerated.

Figure 6: Days Since Entering Prison



Notes: Results based on the population of male prisoners convicted of crimes in Oregon committed after June 30, 2009 and before July 1, 2013. We include all prisoner-days from entry until the earlier of the prisoner's release and June 30, 2014. In Panel A, we plot days served for each prisoner. In Panel B, the sample is limited to prisoners serving consecutive six-month review periods.

Table 19: Prisoner Behavior Around 6 Month Review Cycles: Groups B, C, and D

	(1)	(2)	(3)	(4)
New Review Period	0.00018	0.00008	0.00001	0.00001
	(0.00027)	(0.00028)	(0.00030)	(0.00030)
Days Until Review	-0.00055	-0.00014	-0.00010	-0.00011
	(0.00037)	(0.00037)	(0.00039)	(0.00039)
Days After Review	0.00018	-0.00001	0.00005	0.00006
	(0.00054)	(0.00054)	(0.00055)	(0.00055)
Regime (=1 if 30%)		-0.00017	-0.00005	-0.00005
		(0.00014)	(0.00018)	(0.00018)
Days Served		-0.00000***	-0.00000***	-0.00000***
		(0.00000)	(0.00000)	(0.00000)
Days Served Squared		0.00000***	0.00000*	0.00000*
		(0.00000)	(0.00000)	(0.00000)
Black		0.00216***	0.00215***	0.00215***
		(0.00031)	(0.00040)	(0.00040)
Hispanic		-0.00038*	-0.00110***	-0.00110***
		(0.00022)	(0.00027)	(0.00027)
Other Race		0.00046	0.00023	0.00023
		(0.00041)	(0.00043)	(0.00043)
Observations	1,905,329	1,905,329	1,905,329	1,905,329
Mean Misconducts	0.00417	0.00417	0.00417	0.00417
Prisoner Controls	No	Yes	Yes	Yes
Facility × Month FE	No	No	Yes	Yes
Facility × Day-of-Week FE	No	No	No	Yes

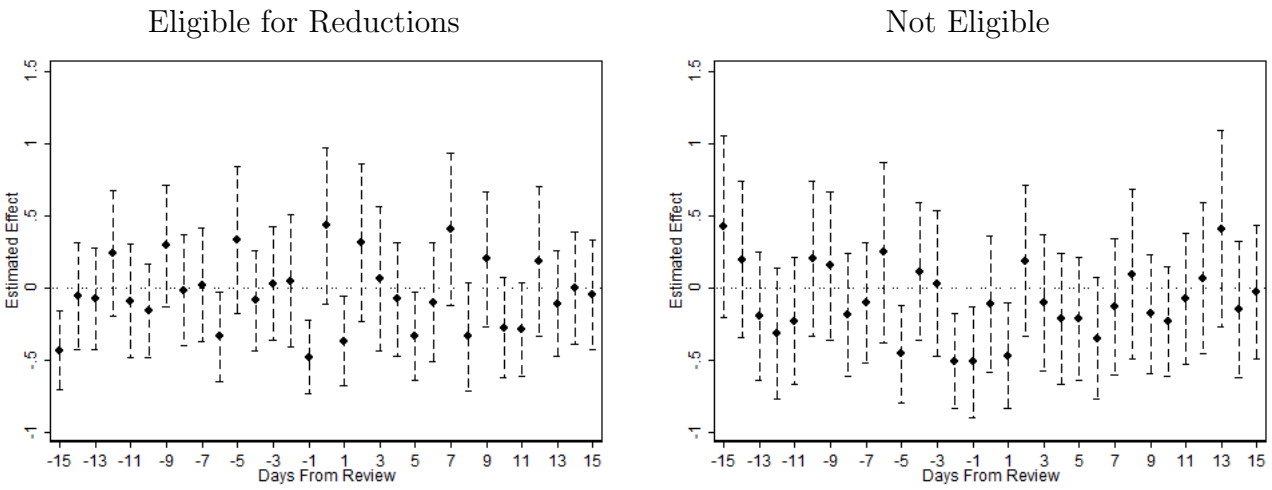
Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the facility-month level and are reported in parentheses. Only prisoner-review cycles in which a prisoner experienced at least 89 days both before and after a review are used to estimate these results. Shortened review cycles occur due to prisoner release.

Table 20: Prisoner Behavior Around 6 Month Review Cycles - By Group

	Group A (1)	Group B (2)	Group C (3)	Group D (4)
New Review Period	-0.00049	0.00084	0.00000	-0.00006
	(0.00035)	(0.00112)	(0.00054)	(0.00038)
Days Until Review	-0.00003	0.00137	0.00011	-0.00040
	(0.00049)	(0.00128)	(0.00073)	(0.00052)
Days After Review	0.00118*	-0.00231	-0.00029	0.00062
	(0.00068)	(0.00213)	(0.00102)	(0.00071)
Regime (=1 if 30%)			-0.00187***	0.00021
			(0.00047)	(0.00024)
Days Served	-0.00000***	-0.00000	-0.00000	-0.00000***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Days Served Squared	0.00000**	-0.00000	0.00000	0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Black	0.00081*	0.00763***	0.00329***	0.00053
	(0.00042)	(0.00237)	(0.00059)	(0.00043)
Hispanic	-0.00084**	-0.00041	-0.00048	-0.00125***
	(0.00042)	(0.00107)	(0.00048)	(0.00032)
Other Race	0.00013	0.00178	0.00039	-0.00033
	(0.00049)	(0.00145)	(0.00080)	(0.00057)
Observations	1,640,805	139,959	699,584	1,065,786
Mean Misconducts	0.00448	0.00364	0.00443	0.00407
Prisoner Controls	Yes	Yes	Yes	Yes
Facility × Month FE	Yes	Yes	Yes	Yes
Facility × Day-of-Week FE	Yes	Yes	Yes	Yes

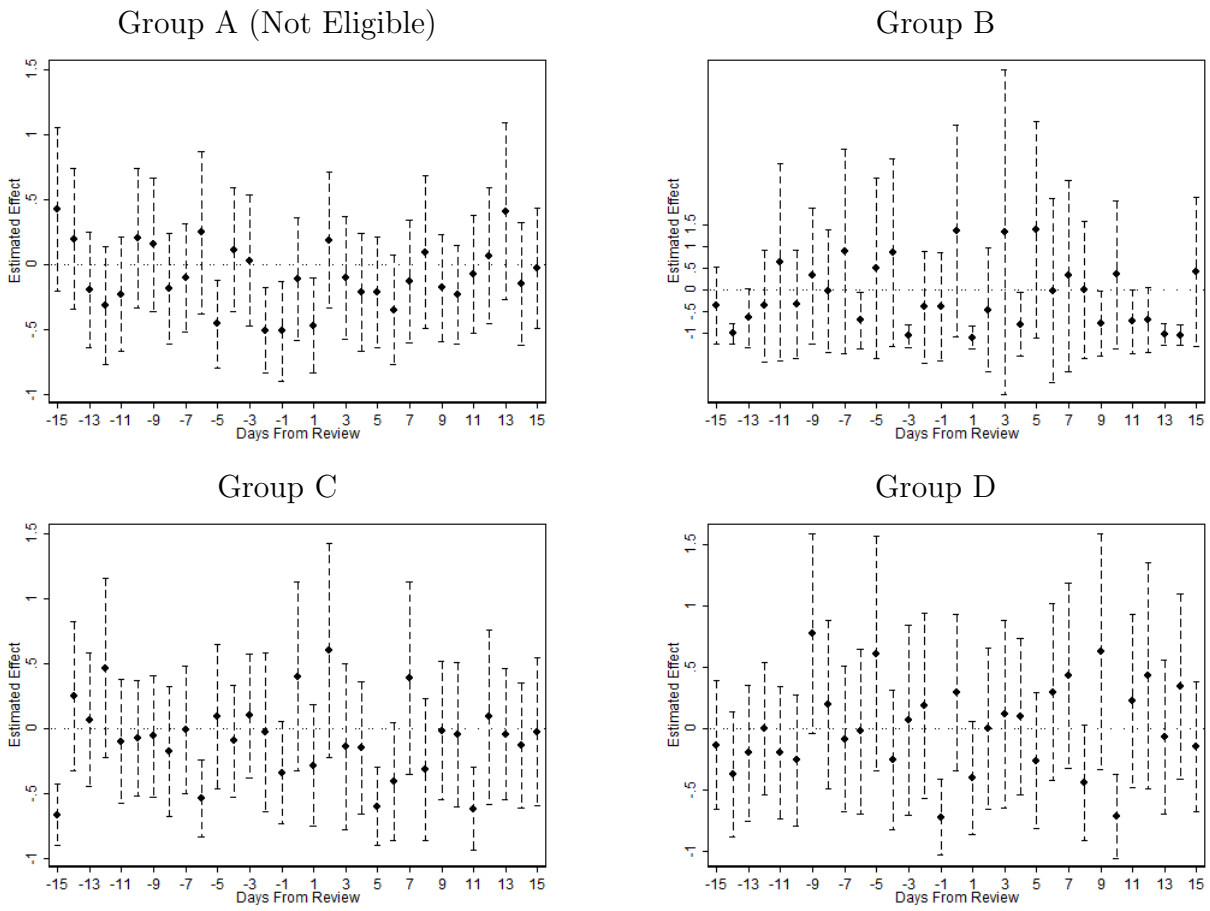
Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors allow for clustering at the facility-month level and are reported in parentheses. Only prisoner-review cycles in which a prisoner experienced at least 89 days both before and after a review are used to estimate these results. Shortened review cycles occur due to prisoner release.

Figure 7: Prisoner Misconducts in the Days Immediately Adjacent to a Review



Notes: Each point represents the days-from-review fixed effect estimate for days ranging from 15 days before a review to 15 days after, as per equation (5). The tails of each point estimate represent the 95-percent confidence intervals, allowing for clustering at the facility-month level.

Figure 8: Prisoner Misconducts in the Days Immediately Adjacent to a Review - By Group



Notes: Each point represents the days-from-review fixed effect estimate for days ranging from 15 days before a review to 15 days after, as per equation (5). The tails of each point estimate represent the 95-percent confidence intervals, allowing for clustering at the facility-month level.